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Right Ascensions of stars observed with the Cape Transit Circle towards the Northern Horizon require a small negative correction.

This is exactly the opposite result which is derived from a comparison with other catalogues. This discordance may be due to change in the collimation of the Transit Instrument at different altitudes, and which cannot be eliminated in a non-reversible instrument, or it may arise from lateral refraction produced by the non-symmetrical arrangement of the building outside the shutter-opening.

Both these possible sources of error will be eliminated in the construction and installation of the new Transit Circle.

Note on Dr. Gill's Paper, "On a New Instrument for Measuring Astrophotographic Plates" (Monthly Notices, lix. p. 61). By H. H. Turner, M.A., F.R.S., Savilian Professor.

In the last number of the *Monthly Notices* Dr. Gill describes a new instrument, constructed by Messrs. Repsold, for measuring photographic plates, which he considers an improvement on the instruments in use at Greenwich and at Oxford.* The main features of the Greenwich and Oxford instruments are :—

(1) Rectangular coordinates of the stars on the plate are obtained by referring each star image to the four réseau lines immediately surrounding it.

(2) The x and y coordinates of an image and the size of its disc are measured at the same time. [In some other instruments only one coordinate is accurately measured at one time ; the plate is then rotated through 90°, and the other coordinate is measured. There are risks of mal-identification with this method, and it takes a longer time.]

(3) The coordinates are measured by means of *scales* in the eyepiece of the microscope, reading to 0·05 mm., and by estimation to 0·005 mm.

(4) It may be added, as a matter of detail, but a thoroughly important one, that the plate is measured twice in reversed positions (turned through 180°), which determines or eliminates personality in measurement and is a valuable check on mistakes.

Dr. Gill proposes no change in (1), (2), and (4), but substitutes for the scales mentioned in (3) two micrometer screws at right angles. He claims that such screws give a much greater accuracy in pointing on the star images, and I will concede him this point, which amounts to admitting that the *accidental error* of a pointing is diminished about one-half. But consider at what a cost this advantage is obtained !

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* For description of the Oxford instrument see *Monthly Notices*, lv. p. 102.

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(*a*) *The Introduction of Systematic Errors due to Wear of Screws.*

Immediately following Dr. Gill's paper on the new instrument there is another from his own pen on the wear of the micrometer screws of the Cape Transit Circle during ten years. From his annual reports I find that during those ten years about 50,000 observations were made with these screws, and they have worn so as to exhibit errors amounting to about $0''\cdot3$. The screws which he proposes to use in his new measuring instrument have precisely the same* work to do as those which read the microscopes of a transit circle, viz., to travel backwards and forwards to a point arbitrarily placed on a space of $5'$; and hence we may expect similar wear after 50,000 bisections have been made with them. But to measure his plates for the Astrographic Chart, Dr. Gill's screws will have to make ten or a dozen times this number of observations! At this Observatory during the last year alone 155,000 measures have been made, and if we had used screws they would have had three times the wear of Dr. Gill's transit circle screws; whereas our scales are untouched by wear. If they have systematic errors, these have presumably remained the same throughout, and can be determined by the observations themselves, and then the corrections applied throughout, if thought advisable. But screws would have been gradually wearing all the time, and if the errors at the end of the year were measured some hypothesis would have to be made for applying the troublesome correction depending on the time.

(*b*) *The Introduction of Casual Errors due to Curvature of the Plate.*

The use of scales in the eyepiece allows of the "error of runs" being checked at each observation. Dr. Gill adjusts the "runs" (*i.e.* the correspondence of ten revolutions of the screw to one réseau interval), once for all, for any given plate. He says (p. 66):—

"By means of these two screws it is very easy to adjust the micrometer so that the images of the sides of réseau square fit systematically between the parallel webs of the fixed square. This adjustment once made is not liable to change, but on account of shrinkage of the film and division error of the réseau it is never found that all the images of the réseau-squares of any plate exactly fit the fixed square."

"Shrinkage of the film and division error of the réseau" are not, however, the only or (I believe) the chief sources of variation. There is also curvature of the plate, for the plates are by no means flat; and an error due to this cause, while it can be

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* There are ten revolutions to the $5'$ space for the new instrument, instead of five revolutions, as in the transit circle. But I do not think this will make any difference. On the one hand, the value of a given fraction of a revolution in arc is just halved; but the number of turns to cover any given space, *i.e.* the wearing effect, is just doubled.

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promptly corrected when a scale is used, remains uncorrected by Dr. Gill's method, if I understand it rightly. (On this point see also *Monthly Notices*, lv. p. 104.)

(*c*) *The Large Amount of Preliminary Adjustment required.*

This can be seen by reading Dr. Gill's paper. With the scales there is practically none. It does not even matter if the scales are oblique to the réseau lines; though it would introduce serious systematic errors if the webs moved by the screws were oblique, and the setting of these accurately involves time and labour. Any accidental breakage, &c., necessitates the repetition of much labour.

(*d*) *The Great Expense of the Instrument.*

In some respects this is an unimportant matter; the capital outlay is, in any case, small compared with the total cost of the work to be done. But an expensive instrument cannot, for instance, be ordered in wholesale quantities. We have found it an immense convenience here to have four instruments (total cost £120), one or more of which may be taken home on special occasions, or lent to a measurer who had spare time but could not attend at the observatory, or lent for experiment to other observatories.

(*e*) *Convenience of the Observer.*

A scale in the eyepiece is read without removing the eye, so that no change of focus is necessary. To read the graduations of a micrometer screw, on the other hand, the head must be withdrawn; and there is a tendency to withdraw it as little as possible, slightly altering the focus of the eye at the same time. This is a comparatively tiring operation. I notice that Dr. Gill's measurer changes places with the recorder every quarter of an hour or so, which seems to support the view that the operation is a little fatiguing. Here a measurer can go on for two, three, or four hours without sensible fatigue over and above that which any continuous occupation necessarily brings. He also records his own measures. For this he has to withdraw the eye from the microscope, but the record-sheet is easily placed, after a little trial, in such a position that his eye does not change focus.

On actual rapidity it is dangerous to lay too much stress. In this I quite agree with Dr. Gill. But I have been myself astonished to find what can be done with the Oxford micrometers —150 to 180 stars an hour (which would correspond to 300 to 350 for two people working together), and this kept up for four hours! It was only recently that I found out what could be done in this way: my bargain with the measurers was that they should account for fifty stars an hour when we took stock of the week's work every Monday morning; this, I thought, would enable them to do the work well and carefully. The comparison of measures, reverse and direct, was a constant check on the carefulness, of which I found no reason to complain; and yet some of them were measuring at two or three times this rate

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when so inclined, enjoying the pleasures of idleness or conversation with the time saved. This frank avowal may cause some amusement, but it may also be of assistance to others in similar case.

(*f*) *Increase of the Numerical Work.*

We limit our work at Oxford to *three decimal places*, *i.e.* our unit is 0″·3, except for the computation of "standard coordinates" from the meridian observations, which has been done to four. [I am not at all sure it would not have been better to work to three only; but the work was arranged before we were bold enough to make this limitation.]

The increase of work involved in going to four places (0″·03) instead of three is enormous; and since the measures are only made to 0″·3, I have not considered it worth while to correct them so elaborately. But if we reduce the probable error of the measures to Dr. Gill's standard, three places is scarcely enough, and this great increase of numerical work is necessary. The increase is not in the ratio of four to three, but in a ratio more like two to one. For instance, in dealing with the differences between measures and calculation, these differences may all be less than ten of the larger units, and expressible therefore by single figures, and it would then take double the number of figures to express them in the smaller units. Consider, too, the difference in labour involved in adding up the columns below :—

	Three Figures.	Four Figures.
$x-x^1$	+·003	+·0028
ax	− 6	− 59
by		+ 04
c	− 23	− 232
	−·026	−·0259

I fear, however, some will regard this economy of figures as false economy. I can only hope it will be given a more extended trial. I do not think those who try it will go back to the older plan of painful accuracy in computation.

To show how little real accuracy is lost by dropping the fourth figure, I give an extract from what we have named the "ledgers," books in which the Cambridge (*Ast. Gesell. Zone Cat.*) meridian places are compared with the Oxford photographic measures on various plates. The results obtained for the same star from different plates—taken at different times and with different centres—are a better test of real accuracy than the accordance of two pointings on the same star image. I have given the results in seconds of arc as a more familiar unit than our unit $0^{\mathrm{R}}{\cdot}001 = 0''{\cdot}3$. The extract is made quite at random, except that it is taken from a part of the sky where there are many stars.

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Corrections to Cambridge Zone Catalogue from Photographic Measures.

Cambridge.								Oxford.						
Number	Mag.	R.A. 1900·0.			N. Dec. 1900·0			Corrections.		Plate Number.	Plate Centre.			Date.
								x.	y.		R.A.		N. Dec.	
		h	m	s	°	′	″	″	″		h	m	°	
11001	8·9	20	8	35·37	27	46	24·7	+1·5	0·0	1105	20	12	27	1897·630
								+1·5	0·0	1113	20	4	27	7·671
								+0·6	+0·3	463	20	6	28	3·695
11002	9·5	20	8	39·39	25	26	40·3	+1·8	−0·9	867	20	12	25	1895·745
								+1·8	−0·9	126	20	8	26	2·613
								+1·5	−0·3	1121	20	8	26	7 690
11003	9·5	20	8	38·67	29	15	51·2	not yet measured						
11004	8·9	20	8	46·50	29	59	36·5	not yet measured						
11005	7·9	20	8	48·78	26	59	0·3	+1·5	0·0	126	20	8	26	1892·613
								+1·2	0·0	1105	20	12	27	7·630
								+1·8	0·0	1113	20	4	27	7·671
								+2·4	+0·3	463	20	6	28	3·695
								+1·2	+0·3	1121	20	8	26	7·690
11006	9·5	20	8	50·36	28	54	27·0	0·0	−1·8	463	20	6	28	1893·695
11007	9·5	20	8	52·17	29	17	50·3	not yet measured						
11008	9·3	20	8	57·02	25	14	39·2	−1·5	−0·3	867	20	12	25	1895·745
								−1·2	−0·6	126	20	8	26	2·613
								−1·5	−0·6	1121	20	8	26	7·690
11009	9·2	20	8	58·16	29	5	4·9	not yet measured						
11010	8·8	20	9	1·73	25	13	13·7	−0·6	−0·9	867	20	12	25	1895·745
								0·0	−0·3	126	20	8	26	2·613
								0·0	−0·6	1121	20	8	26	7·690

Results such as these show that the places obtained are sufficiently accurate; while the addition of another decimal place to the residuals, in the case of No. 11005, for instance, is clearly of no interest. Far better spend the time needed to obtain it in taking and measuring another plate.

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A Suggestion for the Explanation of Stationary Radiant Points of Meteors. By H. H. Turner, M.A., F.R.S., Savilian Professor.

1. For many years past Mr. W. F. Denning has insisted that there are radiant points of meteors which remain fixed in the same portion of the sky for several months together. Indeed, he is disposed to believe that the Earth is liable to receive showers from the same radiant at all points of her annual orbit. It has been found difficult to explain this phenomenon theoretically. The conception of a swarm of meteors sufficiently extended to cross the Earth's orbit at many points is not difficult; but, if we assume them to be moving in parallel paths, the radiant, which depends on the relative motion of the swarm *and the Earth*, will shift among the stars during the year, as in the case of the well-known August *Perseids*. It would only remain approximately constant in position if the velocity of the meteors were enormously great compared with that of the Earth, a supposition which will not accord with the direct observations of velocity. Indeed, to quote the words* of Professor C. A. Young :—

"No satisfactory explanation of such fixity (of the radiant) as yet appears; and though Mr. Denning is perfectly confident of the genuineness of his discovery, and though it is very generally accepted as a fact, some very high authorities, Tisserand for instance, still question it, as being 'incredible and unaccountable.'"

2. My attention was drawn to this discrepancy between theory and observation more or less by accident. I know nothing of meteoric observation at first hand, but the confidence with which Mr. Denning speaks of the evidence in favour of stationary radiants seems to me sufficient to inspire others with similar confidence, and I devoted some little time to thinking over the matter. I was ultimately led to consider the effect of the Earth's action on that portion of the swarm which passed near, but did not meet, the Earth. Schiaparelli and others have fully considered the effect of the Earth's attraction in deflecting the paths of meteors which reach our atmosphere and become visible to us; but, so far as I have been able to ascertain, no one has yet considered fully the effect on the rest of the swarm. A brief note on the subject which I submitted to Professor Alexander Herschel received such favourable comment from him that I am encouraged to publish it. The original note was rather hurriedly drawn up, and is not reproduced verbatim, but is given in a revised form in the next five paragraphs.

3. Suppose, in the first instance, that a stream of meteors is

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* Young's *General Astronomy*, edition of 1898, p. 481.